

Rainfall–entrained Marshland Sediment: A Non–point Source of Nutrients and Contaminants to the Estuarine Water Column

Raymond Torres

Public Comments

No public comments were received for this proposal.

Technical Synthesis Panel Review

Proposal Title

#0302: Rainfall–entrained Marshland Sediment: A Non–point Source of Nutrients and Contaminants to the Estuarine Water Column

Final Panel Rating
adequate

Technical Synthesis Panel (Primary) Review

TSP Primary Reviewer's Evaluation Summary And Rating:

This is a generally strong proposal that addresses rainfall–entrainment of marshland sediments and their potential impact on tidal ecosystems. Building from previous work, the investigators hypothesize that marshland sediments are point sources of nutrients and contaminants, and propose to investigate the geochemistry and flux of these sediments in two Bay Area marshlands. The goals and objectives are well stated and supported by a physically–based conceptual model that presents a convincing case for investigation of this issue. The proposed approaches are generally well developed and based on prior experience of the lead investigator, providing a high probability of success. Primary criticisms are: 1) limited number of study sites and samples per site may preclude generalization/extrapolation of results; 2) sampling duration per event not specified; 3) one year of data collection may not be sufficient given that entrainment processes are a function of the intensity and duration of rainfall events; 4) the author has not yet established local research connections in support of this project; 5) there is insufficient citation of relevant local studies; 6) the author did not make a convincing case for the significance of rainfall entrainment and subsequent contaminant flux within the context of other sources and mechanisms of contaminant

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Technical Synthesis Panel Review

flux; and 7) the proposed budget may be too high (CALFED would pay 100% of the lead PI's sabbatical salary while in California). Despite these criticisms, this is a well-crafted study, with potentially significant ecological results and should be considered. The technical reviewer ratings (good and very good) were in general agreement with the rating given here (adequate).

Additional Comments:

This is a generally strong proposal that addresses rainfall-entrainment of marshland sediments and their potential impact on tidal ecosystems. Building from previous work, the investigators hypothesize that marshland sediments are point sources of nutrients and contaminants, and propose to investigate the geochemistry and flux of these sediments in two Bay Area marshlands. The goals and objectives are well stated and supported by a physically-based conceptual model that presents a convincing case for investigation of this issue. The proposed approaches are generally well developed and based on prior experience of the lead investigator, providing a high probability of success. Primary criticisms are: 1) limited number of study sites and samples per site may preclude generalization/extrapolation of results; 2) sampling duration per event not specified; 3) one year of data collection may not be sufficient given that entrainment processes are a function of the intensity and duration of rainfall events; 4) the author has not yet established local research connections in support of this project; 5) there is insufficient citation of relevant local studies; 6) the author did not make a convincing case for the significance of rainfall entrainment and subsequent contaminant flux within the context of other sources and mechanisms of contaminant flux; and 7) the proposed budget may be too high (CALFED would pay 100% of the lead PI's sabbatical salary while in California). Despite these criticisms, this is a well-crafted study, with potentially significant ecological results and should be considered. The technical reviewer ratings (good and very good) were in general agreement with the rating given here (adequate).

Technical Synthesis Panel (Discussion) Review

TSP Observations, Findings And Recommendations:

This is a well-written proposal that addresses a potentially important topic. The proposal seeks to determine if marshland sediments are point-sources of nutrients and contaminants and whether (and to what degree) rainfall is a mechanism for mobilizing nutrients and contaminants held by the sediments. The investigator has considerable prior experience with this topic and will likely be successful. However, external reviewers were concerned that the duration and intensity of sampling may be inadequate. The panel was very concerned that the applicant had not yet established the local research connections he will need to design (e.g. sampling sites) and implement this study. The author did not demonstrate that he had conducted sufficient background research to identify sites for which there is adequate background data on nutrient flux. Also, there is insufficient citation of relevant local studies. Finally, the author did not make a convincing case for the significance of rainfall entrainment and subsequent contaminant flux within the context of other sources and mechanisms of contaminant flux.

Rating: Adequate

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proposal title: Rainfall–entrained Marshland Sediment: A Non–point Source of Nutrients and Contaminants to the Estuarine Water Column

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	The goals of this project are clearly stated. There are three main research questions detailed on p. 3. The proposed research is timely and important. Few studies have examined the effects of rainfall on movement of materials in marshes. However, the authors present convincing data that rainfall is an important process affecting the movement of sediment, organic matter, and contaminants.
Rating	excellent

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full–scale implementation project justified?

Comments	The conceptual model (pp 5–9) is thorough and solidly based in the scientific literature. The model is primarily based on hydrodynamic aspects, and while marsh vegetation is discussed, its role is not directly addressed in the proposed research. The research focuses on processes occurring in tidal creeks that thus integrate effects of marsh vegetation, soil characteristics, and other aspects of marshes that drain into the creeks.
Rating	

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excellent

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	<p>The approach presented has been successfully implemented by the PI in South Carolina marshes, and involves setting up two monitoring stations, one each in a creek in Grizzly and Honker Bay. Each site will include grab sampling and automated monitoring of turbidity, discharge, rainfall, and tidal water level. Elevation of gages and surrounding marsh will be determined. Suspended sediments will be collected and analyzed for carbon, stable isotopes, texture, metals, and organic chemicals. The PI will be in California for a sabbatical (either 6 months or one year—it is not clear in the proposal), and so will be near the sites during setup and at least a large portion of the monitoring. The methods proposed seem rigorous, and the PI seems quite familiar with them. A limitation of the proposed research is that the PI has not yet interacted with USGS and other staff at the Mallard Island OBS station or other researchers and data managers in the Suisun Bay delta area. Additionally, he has not visited specific research sites yet. However, the PI would network with various people during a 4 week period to establish connections and visit sites under the proposal. Another aspect of the research is that the experimental design seems limited in the number of sampling stations, and the number of samples collected and the duration of sampling is not clearly specified. Monitoring stations will be set up in only two locations (duration of monitoring is not clearly stated), and possibly only 7 samples (according to the budget) will be analyzed for metals. No sample quantities are listed for geochemical or</p>
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	physical analyses. Similarly, the number of marsh surface samples and suspended sediment samples is not listed (see p. 12). If analyses are limited to only two sets of 7 samples (which I find hard to believe) then this is far too few to provide a meaningful assessment of particulate and contaminant movement. Similarly, because only two sampling stations will be set up, it may be difficult to extrapolate to other systems, particularly if the two study sites show very different patterns of sediment transport and hydrology. Despite some lack of clarity with experimental design, the results are likely to add to the base of knowledge, and provide information on the role of rainfall in movement of materials in marshes, processes that have not been widely studied. The information may be useful to decision makers if the results of this research can be incorporated into larger-scale models of material flow in estuaries.
Rating	very good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	The methods used are well-documented and seem feasible. The scope of the proposed research is consistent with the ability of the authors to carry it out. To some extent, the success of the project depends on the development of contacts with data managers and other researchers already working in the region, and on the suitability of sites that are chosen.
Rating	very good

Monitoring

If applicable, is monitoring appropriately designed (pre–post comparisons; treatment–control

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comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	
Rating	not applicable

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	The products from this research will include the standard peer-reviewed publications and Power Point presentations, and a website with pictures, graphs, Power Point presentations, and apparently the data from the project. Contributions to larger data management systems are not included, and interpretive outcomes from this project seem unlikely.
Rating	good

Additional Comments

Comments

Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The project team seems highly qualified to carry out the proposed research. The PI has an excellent career record, and has conducted similar research in South Carolina. The geochemist and the subcontractor similarly seem well-qualified to carry out the necessary analyses. Lab space and other resources are available in South Carolina; the availability of lab space or other resources in CA is not mentioned.
Rating	

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	excellent
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Budget

Is the budget reasonable and adequate for the work proposed?

Comments	A major portion of this \$278K budget would go to support the PIs sabbatical in CA. This includes about \$100K salary, 20% benefits, and a 6-month housing allowance, plus 46% indirect cost on these items. One question I have is why is a full year of salary necessary if the PI will only be spending 6 months in CA (if that is the case?). Additionally, half of annual sabbatical salaries are often covered by the researchers' own institution, and the visited institution sometimes covers additional salary. Assuming CALFED policy allows funding for sabbatical salary, I have no problem with it. With the possible exception of \$500 per sample for metal analyses (if I am reading the budget correctly), the other items in the budget seem reasonable.
Rating	very good

Overall

Provide a brief explanation of your summary rating.

Comments	The ideas proposed here are interesting and the methodologies suitable for addressing the research questions. Limited detail on numbers of samples collected and analyzed makes it difficult to fully evaluate the research approach. Additionally, the PI has had limited interaction with parties involved in CALFED-related projects, which may hamper implementation of the research and dissemination of the findings to interested parties in the Bay area.
Rating	very good

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proposal title: Rainfall–entrained Marshland Sediment: A Non–point Source of Nutrients and Contaminants to the Estuarine Water Column

Review Form

Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

Comments	<p>The goals are clearly stated as three Research Questions in the Project Purpose section. These goals are addressed and further outlined in the remainder of the proposal.</p> <p>The idea of measuring the impact of rainfall on marshland sediment and particularly on marshland creek turbidity is scientifically valid. The importance of the proposed research cannot be fully judged prior to its completion, because the relative contribution of rainfall-induced turbidity may prove to be minor or significant. The larger question is whether this type of information is a top priority relative to other research needs in Grizzly and Honker Bays, and I am not in the best position to make that determination.</p>
Rating	very good

Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full–scale implementation project justified?

Comments	<p>The author makes reference to several publications, some in progress, which describe the results of similar research performed on the East Coast of the</p>
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	<p>United States. Several of the postulated hypotheses appear to be based on recent work. Some of the proposed research goals are similar to the previously accomplished goals, and therefore a part of the proposal is aimed at repeating similar field experiments, but under different conditions specific to marshes in Grizzly and Honker Bays. Consequently, the proposed research would add to the existing knowledge on rainfall-entrained sediments and provide site-specific measurements which will be useful to sediment and heavy metal mass balances in the northern reach of the Estuary.</p> <p>A conceptual model is clearly stated and appropriately explained. The proposed scale of the project is also appropriate, though the applicability of the measurements could be even broader if additional sites were selected.</p>
Rating	good

Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

Comments	<p>The approach and methods are well developed. The approach is particularly reliable since the author has performed similar research in marsh sites on the East Coast, and at least some of those results have been published in peer-reviewed journals.</p> <p>Of the three Research Questions, question #2 is not as well addressed in the methods section, and it is not entirely clear how this objective will be accomplished. Specifically, it is not explained how the author proposes to determine sediment transport, or at least over what distance. It is possible that I missed this explanation, but a better approach in the</p>
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	<p>proposal would have been to structure the approach and methods description to map directly onto the research questions.</p> <p>The approach is feasible and, aside from the issue raised above, will accomplish the stated goals.</p> <p>As I stated under "Justification," the proposed research would add to the existing knowledge on rainfall-entrained sediments and provide site-specific measurements. The research would also serve to refine the methodology, especially with respect to the impact on heavy metal and nutrient mobilization.</p> <p>The degree to which the results will be useful to decision makers will depend in large part on whether the natural process under consideration plays a major role in sediment, nutrient, or metal cycling.</p>
Rating	very good

Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success?
Is the scale of the project consistent with the objectives and within the grasp of authors?

Comments	As I stated in "Approach," the proposed approach is feasible. Success is likely. The scale of the project is commensurate with the project objectives.
Rating	very good

Monitoring

If applicable, is monitoring appropriately designed (pre-post comparisons; treatment-control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

Comments	The monitoring design appears sound. Again, the author is drawing on his experience in performing similar measurements on the East Coast, and therefore the
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	design is not completely new. The proposed methods for interpreting the turbidity data are appropriate and thorough. However, the author does not describe in sufficient detail how the trace metal and nutrient data will be analyzed from a statistical and temporal perspective, nor how it will be placed in a larger context. Instead, the author provides a detailed description of analytical methods, which could have mostly been referenced to the literature. Particularly puzzling is the nearly three-quarter of a page description of PAH analyses, because PAHs are not previously discussed in the proposal and are not included in the general description of approach.
Rating	good

Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

Comments	I have addressed aspects of this issue in the previous sections. Generally, the products could prove very useful in the sediment, nutrient, metal (?and PAH) cycling and mass balance in the northern reach of the estuary. Although the relative contribution of the process in question (sediment entrainment) is as yet unknown, a finding of negligible impact would still be scientifically useful and would guide future research in this area.
Rating	good

Additional Comments

Comments	My main concern about the proposed work is whether it will provide only an incremental advancement in knowledge relative to the work performed by the author on the East
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	<p>Coast. For instance, how much of that experience could be re-interpreted and extrapolated to provide a first-order estimate of the relative importance of the rainfall-entrainment process? Is it possible to arrive at a reasonable range of sediment entrainment rates and thereby be able to judge the relative importance of this process in Grizzly and Honker Bays? One should note that the large sediment concentrations cited by the author as being a result of rainfall entrainment in the East Coast studies were obtained by simulating rainfall events with a 2-10 year recurrence interval. One would assume that the smaller the recurrence interval, the less significant the entrainment process. It follows that one year of data collection, as proposed by the author, may not capture rainfall events of this magnitude.</p> <p>Finally, I suggest the author add selenium to the suite of metals. Selenium is one of the major environmental concerns in the San Francisco Bay and the added cost of analysis would be minor.</p>
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Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

Comments	The author and his collaborators all have solid track records in their respective fields. It appears that the team is well put together and capable of the task at hand. All support mechanisms and the overall infrastructure seem sound.
Rating	very good

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Budget

Is the budget reasonable and adequate for the work proposed?

Comments	I have two concerns about the budget. First, the overall cost seems high. Upon closer inspection, the distribution of labor (my other concern) appears to be the reason for high costs. The proposed budget provides nearly 10.5 months of direct salary for the PI. On the other hand, it provides only 7.5 months of salary for a graduate student. Given that the nature of much of the field work, and particularly Tasks 5 and 6, is well suited to a graduate student, I believe too much labor was budgeted for the PI. However, the two tasks that I believe are overbudgeted are Tasks 8 and 10. Under Task 8, nearly \$46,000 was budgeted to perform time series analyses on the rain-turbidity-tide data. This includes 2.75 months of the PIs labor and an additional 1.5 months of graduate student labor. This is excessive, in particular in terms of the PI labor. Similarly, nearly \$50,000 was budgeted for the preparation of manuscripts, including 2.5 months of labor for the PI and 2 months for a graduate student. This is also excessive. I believe the costs of Tasks 8 and 10 could each be halved, resulting in a net reduction of the overall budget by approximately \$50,000.
Rating	fair

Overall

Provide a brief explanation of your summary rating.

Comments	With the exception of my previously stated concerns, I believe the proposed work has value and the results would likely be applicable to models of sediment, nutrient and metal cycling in the northern reach of the San Francisco Estuary. The proposed work should be evaluated
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	with respect to its applicability to regional needs, relative to other proposals in this area.
Rating	good

